

appending any criticism on the admissibility or otherwise of this analogical piece of reasoning, we will simply narrate the results of putting the question to the test of experiment. When oxalate of lime was deposited in a gelatin plug between the poles of horseshoe magnets, "there was an extraordinary increase in the size of all the forms, crystalline and non-crystalline, where the plug or gelatin was subjected to the action of magnetism, but there was no production of new forms or greater tendency to sphericity." Similar experiments with a large electromagnet yielded crystals which in several cases *appeared to have their axes slightly twisted*. This observation, if confirmed, and if presenting any assignable relation between the direction of magnetisation and that of the alleged axial twist, would be in the highest degree interesting. Up to the present moment, so far as we are aware, no crystal presenting tetrahedral dissymmetry or optically active in the polarimeter has been procured by artificial synthesis. Is it possible that Dr. Ord's observation contains the germ of the method by which we may hope to procure the synthesis, not of the active tartrates and sugars only, but of quinine and other alkaloids also? Experiments with electric currents were also tried, but proved less satisfactory, though the electrolytic actions set up produced several unexpected results.

Later chapters in Dr. Ord's book are devoted to renal and biliary calculi other than those mentioned—including a very singular case of an indigo calculus—and to a short scheme for the qualitative examination of calculi, which contains valuable hints to the general practitioner.

Concerning the production of the collospheres themselves there does not appear to be any one assignable cause. Harting dwells strongly on the influence of the "nascent" state in which the crystalloid body is deposited by double decomposition within the colloid. This term will probably fall out of use by chemists so soon as they perceive that it is a term convenient only as a cloak for ignorance. A more satisfactory point is made by Dr. Ord in the suggestion that there exists a relation yet undiscovered between hydration and the colloidal state; the hydrate of fresh uric acid being a colloid. Dr. Ord is of opinion that hydrated colloids and strong solutions of very soluble salts alike prolong the colloidal state of certain crystals, thus favouring the production of spheroids. Dehydration, which in certain cases appears to determine the production of spheroidal forms, is obviously inadmissible as the cause in the majority of cases. Nor does the difference of crystalline form between one crystal-system and another appear to affect the collospheric condition, in which absolutely no smallest modifications attributable to this possible cause can be detected. Solubility undoubtedly has much to do with the matter, since insoluble crystalline substances yield the best spheroids; but by evaporation and by deposition from hot strong solutions even sulphate of copper and ferrocyanide of potassium can be thus obtained. We must therefore fall back upon the conclusion that the one important factor in the production of the collospheric condition is the influence of the colloid. Mr. Rainey, who came to this conclusion, attributed this action to the "viscosity" or tenacity of the colloid fluid; and hence he associates with true colloids such substances as glycerine (which is a true crystalloid) and other viscid substances.

Dr. Ord, on the other hand, is disposed to regard the influence of the colloid as "a result of intestinal molecular movement inherent to the constitution of the colloid."

Arrived at this point, however, we cease to perceive any definite coherence between the various speculations which follow and in which the effects of pressure, of strain, and of hypothetic spiral waves, are mixed up with Brownian movements and chemical interaction. It is a pity that the all-important bearing of surface-tension at the boundary of two media, and of the elegant and instructive researches of Plateau, including his production of liquid spheroids, is not once alluded to, even in the remotest manner, by Dr. Ord. For our own part, we are disposed to attribute a very large portion of the influence which determines the production of these collospheres of solid matter to the same molecular actions as those which produce the surface-tensions between solids and liquids, and which cause the rise of liquids in capillary tubes and the production of liquid spherules in the experiments of Plateau.

In conclusion we must not omit to quote one experiment of Dr. Ord, that in which the rapid production of the collospheres is conducted under conditions suitable for lecture demonstration. A solution of pure urea of density 1.026 usually throws down shining white flakes of nitrate of urea on the addition of an equal bulk of strong nitric acid. If, however, a little egg-albumin be added to the urea solution before the nitric acid is added, spheres are formed of the greatest beauty, and appear "floating like snowballs" in the yellowish liquid.

OUR BOOK SHELF

A Guide for the Electric Testing of Telegraph Cables. By Capt. V. Hoskiær. Second Edition. (London: E. and F. N. Spon, 1879.)

THIS very unpretentious but very useful little manual has reached a second edition, and now reappears with several valuable additions. In his original preface the author states that he does not expect an electrician to discover anything new in its pages. Be that as it may, the electrician will acknowledge the debt he owes to Capt. Hoskiær for the precision and brevity with which all his directions concerning the practical details of testing are given. Without philosophising or going into mathematical reasons of why and wherefore, he gives the necessary formulæ in the shape most useful for practical calculations; and the necessary tables of logarithms, trigonometrical functions, and temperature coefficients are sufficiently complete to save reference to other more extended works. The twelve lithographed diagrams leave nothing to be desired in point of clearness.

LETTERS TO THE EDITOR

- [The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.]
- [The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

The Antiquity of Oceanic Basins

IT seems to have escaped Dr. Carpenter's notice¹ that, in a Report on the results of the Deep-sea Dredgings of Mr. Pourtales

¹ Lecture before the Royal Institution, January, 1880.

for 1866, 1867, 1868, Prof. Agassiz¹ had already called attention to the probable great antiquity of the oceanic basins.

Dr. Carpenter seems also to have overlooked the series of physical observations of the depths of the sea commenced by the United States Coast Survey² in 1850, and carried on without interruption to the present day.

The statement made by Mr. Wild³ that the deepest sounding of the *Tuscarora* is not trustworthy, because "no sample of the bottom was brought up," is apparently endorsed by Dr. Carpenter, who says: "The sounding wire of the United States ship *Tuscarora* twice broke without reaching bottom . . . at depths considerably exceeding 4,000 fathoms." This should be modified by stating that the *wire broke while reeling in* twice, once the bottom was not reached, and five casts were made over 4,000 fathoms, bringing up each time a specimen of the bottom.⁴ Capt. Geo. E. Belknap, of the *Tuscarora*, says,⁵ speaking of the casts beyond 4,000 fathoms in depth: "The wire parted at the last two and deepest casts. . . the result of momentary carelessness on the part of the men at the *reeling-in wheel*."

The method of sounding with wire has now been in use long enough to show that even if the *Tuscarora* had not brought up a single specimen of the bottom during her whole trip, and if the wire had *invariably broken while reeling in*, we could not for that reason alone have rejected those soundings as inaccurate.

Those who have sounded with wire know that the instant the sinker has touched bottom is recorded on deck, and the precise depth is then known, whether the cylinder is brought up or not. There is no more reason for rejecting the deepest sounding of the *Tuscarora* of 4,655 fathoms than for rejecting the 480 other casts which are accepted because a bottom specimen came up.

Cambridge, Mass., April 5

ALEXANDER AGASSIZ

On the Alum Bay Flora

IN the list of fossils appended to the paper upon the Alum Bay flora, brought before the Royal Society by Baron von Ettingshausen and reported in *NATURE*, vol. xxi. p. 555, the new species have Ett. and Gard. attached to them, implying that Ettingshausen and myself are their authors. It is only fair to Ettingshausen to state that I had no share in making the determinations, and to myself, that I accept them simply as provisional. Associated as he is with me in the work upon the British eocene floras, he felt that he could hardly publish preliminary work connected with it in any other way. I completely disagree with him, however, as to the utility of publishing new specific names unaccompanied by drawings or descriptions of any kind, and think that a simple list of genera, with the number of new species in each, would have been unattended with any inconvenience. He appears to me to attach altogether undue weight to mere priority in nomenclature, and the existence of such provisional lists, far from aiding research, must prove a serious difficulty to our fellow workers. In the highly probable event of an author being unable to come from some distant country to examine the specimens themselves, is he, for instance, to forbear naming every undescribed species of such common Tertiary genera as *Ficus*, of which eight new and unpublished species are in the list, of *Celastrus*, of which there are five, or of any other of the some fifty genera containing new specific names? He could not safely name even any indeterminate leaf or fruit, for fear it might be one of the long list of Phyllites or Carpolithes for which Ettingshausen has devised specific names.

¹ *Bulletin of the Museum of Comp. Zoology*, 1869, vol. i., No. 13.

² Coast Survey Reports, 1850 to present day; also Bibliography of Biological Results (*Bull. Mus. Comp. Zool.*, vol. v., No. 9, 1878).

³ "Thalassa," 1877, p. 15.

⁴ "Deep-Sea Soundings in the North Pacific obtained by the United States ship *Tuscarora*" (Washington: Hydrographic Office, 1874, No. 54, p. 30).—

1874.	Fathoms.	
June 11	4,643	Wire broke; bottom not reached.
" 17	4,340	Yellow and clay brown mud.
" 17	4,350	Yellowish mud and sand and specks of lava.
" 18	4,041	Yellow and clay-coloured mud and gravel.
" 18	4,234	Rocky; point of cylinder came up battered.
" 18	4,120	Yellow and clay-coloured mud mixed.
" 18	4,411	No specimen; wire broke (<i>while reeling in</i>).
" 19	4,655	" " "

⁵ *United Service Magazine*, July, 1879.

But were our supposititious author to go on with his work, in spite of this "sword of Damocles," would Baron Ettingshausen claim priority and deprive the man who had first figured and published descriptions of them, of the pleasure of christening them in accordance with his views and wishes? If not, *cui bono*?

To show the purely provisional light in which the list must be regarded, I may mention that, unfortunately just as the Baron left England, a large collection, that of the late M. Watelet from the Grès du Soissonnais, came into my possession, and seems, on a cursory examination, to contain a preponderance of species identical with those of Alum Bay. None of Watelet's published species appear in the list of the Alum Bay flora, which therefore must of necessity be considerably modified to include them. The same may be said of the flora of Gelinden, of which a large series has also reached me.

Again, even in the only section of plants yet worked out by us for the palæontographical memoir, the ferns, discrepancies occur. Two ferns occur in this Alum Bay list which are not included in our fern flora from that locality. These are inserted on the authority of Heer, who states that he has seen them from Alum Bay; but as on the occasion of that gentleman's visit or visits to England many years ago the floras from the different localities had not been systematically collected, and were generally mixed together in museums, in the same drawers and cases, and cannot always be identified by the matrix, I prefer to adhere to the opinion of that indefatigable collector, Henry Keeping, who lived within a short distance of Alum Bay, and to my own, Mr. Mitchell's, and all other workers' experience, that no fern but *Marattia* is found there. At all events, if they are to be included in the Alum Bay flora, they should be so with reserve, especially as Prof. Heer's ideas as to the position of the localities and their ages are so hazy that he puts the Alum Bay leaves in the "Bartonisem" (above, if anything), or about 1,000 feet too high, and thinks that Bournemouth is somewhere in the Isle of Wight.

An illustration of the inconvenience caused by publishing names without proper figures and descriptions occurs to me. Heer named a small fern fragment which he supposed to be from Alum Bay, *Asplenium martinii*. This name has got into works by Saporta and Crie, who have each tried to fit ferns of their own into Heer's meagre description. Neither had seen the original, nor could they give any information, and it was only after several attempts to obtain it that Ettingshausen received a rough sketch from Heer showing conclusively that the "species" in question was a fragment of the abundant and well-known *Anemia subcretacea* of Sézanne. I do not even now know whether it was upon this fragment or some other that Heer wrote that he had "seen this form" (*Anemia subcretacea*) from Alum Bay.

J. STARKIE GARDNER

Negritoes in Borneo

HAVING had inquiries addressed to me as to the existence of a Negrito race in Borneo, I think it may be useful to recall attention to, and possibly save from oblivion, a statement on this subject which was published by Windsor Earl in the *Journal of the East Indian Archipelago*. Mr. Earl says that a Capt. Brownrigg, who had been shipwrecked on the east coast of Borneo, informed him (*J.E.A.*, No. 9) that he had lived several months at a town some distance up the Berau River, and that during his stay the town was once visited by a small party of men from the interior, "who must have been of the Papuan race" (*sic*). He described them as being short, strongly-built people, black in complexion, with hair so short and curly that the head appeared to be covered with little knobs like peas; and with many raised scarifications over the breast and shoulders. He described them as being on good terms with the people of the town, mostly Bugis, and as supplying them occasionally with jungle produce.

Of this account it may be remarked that Mr. Earl would not have retailed it unless he had had some confidence in the credibility of his informant—that, so far as it goes, it is curiously circumstantial—and that these people are said to have come exactly from that district in Borneo where we might expect *a priori* to find Negritoes if they existed at all.

Whilst on the subject of Borneo, may I suggest that ethnologists should make a more sparing use of the term "Diak" when treating of the Malay Archipelago? It should only be applied to tribes who themselves use it as the distinctive appellation of their people. As more than one tribe so uses it, there should always be prefixed some word still further limiting its applica-